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AUTHOR Stigliano, Tony
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Is a Scientific Classification of Educational (Behavioral) Objectives Possible?

by Tony Stigliano

People's Computer Company
Saybrook Institute

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Abstract

Robert M. W. Travers's 1980 essay is the focal point of this paper. He argues that evaluation research requires a scientific taxonomy of human behavior and learning. Such a taxonomy must be experimentally based, mathematically expressed, theoretically sound, and predictive. He bases such criteria on the development of scientifically successful taxonomies, specifically, the Periodic Table of the Elements. Unscientific taxonomies are either metaphysical (Aristotle) or standardized practices (Bloom). But, Travers has misread his own historical example; he cannot eschew both metaphysics or human practices. Moreover, his example of a potential source for a scientific taxonomy, Piaget's cognitive science, is rife with problems. The paper concludes with reasons for the unrealizability of any attempt to scientifically describe human actions outside of historical situations.

Introduction

One of the most important theoretical problems faced by evaluators of educational practices concerns the identity and nature of the relevant behaviors being measured or observed. The lack of a metatheory of educationally related behaviors entails the lack of a common language classifying types of learning behaviors. Therefore, there is a limit placed on the possible development of the scientific nature of evaluation research. As we shall see, this is a problem plaguing all of social science research with dire consequences.

Robert Travers has written an important paper (Travers: 1980) which sets out in a programmatic way the conditions required for the development of such a common language and a supporting theory of classification. While Travers does not provide a straightforward list of what might be criteria for such a language, his historical analysis of the development of taxonomies in science and philosophy provides us with an implicit list. Travers rejects certain theories of classification, like Aristotle's, as being too metaphysical. That is, Aristotle's theory was derived from non-observable essences. He adheres rather to scientists like Linnaeus, Newton, and, especially, to Mendeleyev, who created taxonomies which have been accepted by most scientists. From these examples we may abstract the following criteria for a scientific classification of human behavior: (1) classifications must be systems of measurement; (2) they must have the support of a theory or a

law; (3) empirical content must be based on substantial experimentation; (4) future classes or taxa should be predictable from known ones. Taken together, these would be characteristics of a scientific taxonomy of human behavior. Once such a taxonomy is developed, evaluation would have "a sound basis for classifying behavior" and "value judgements through which [learning] behaviors are judged to be worthy" would be clearly related to scientific reasoning (Travers, 1980: 5).

With such a classification system in place, evaluation research--indeed, social science in general--would begin to show the characteristics of a "mature science." Clearly, Travers's essay points in larger, more grand, directions. The need for such a classification of human behavior is becoming more pressing. The social sciences in the past decade or so have clearly fallen on hard times. In the Chronicle of Higher Education for September, 1984, we read that "We face grave peril; we face the undoing of American social science" because no stable, central basis of knowledge has been established for the social sciences which could support theories and laws. This pronouncement comes from the National Science Foundation's senior associate for social and behavioral sciences, Dr. Otto Larsen. Thus, Travers's discussion of human behavioral classificatory systems resonates with the potential delegitimation of the social sciences as science. His argument is significant for anyone interested in finding a way out of the quandary which seems to undermine both the integrity and authority of

research on human behavior and the possibility of controlling human behavior for desired ends.

In this paper, I will argue that Travers's paper is fundamentally confused. Some of this confusion shows up in his inability to decide what the cognitive status of such a classificatory system would be. Other difficulties emerge from his depiction of what, for Travers, are paradigm examples of such systems. But the underlying problem lies in the premiss that a formal, rule-like set of universal propositions (expressed as theory) is the best way any description can be made.

In order to show all this, I have structured this paper in the following way. First, I will elucidate and criticize the assumptions Travers makes about scientific language. Next, I will present a discussion of what Travers takes to be a paradigm example of scientific classification, Mendeleyev's Periodic Table of the Elements. Travers's own suggestion for the social sciences is Jean Piaget's genetic epistemology. This position is itself rife with ambiguities begging the question of the nature of rules and what it means to "follow" them. Last, I shall argue that scientific classifications of behavior may be impossible because human behavior cannot be predicted.

1. Confusions in Travers's Argument

Travers wishes to avoid two extremes in his discussion of classifications and what makes them scientific. On the one hand, he finds Aristotle's metaphysical assumptions too narrow

for a scientific approach. Aristotle, while making important contributions, such as the idea of a hierarchy, assumes that whatever it is that makes something what it is and not something else is an unchanging "essence." Such an essence transcends the empirical. For Aristotle, the definitions and criteria which distinguish one item from another inheres somehow in the substance of the given item. Human rationality, for example, is the distinguishing characteristic which makes (in a causal sense) human different from animal. Rationality is apotheosized as a soul, permanent as the form of human. In effect, metaphysical considerations would predetermine what the scientist would be discovering.

On the other hand, Travers finds Benjamin Bloom's taxonomy of behavioral objectives far too broad in scope and lacking in a genuine scientific basis. Bloom's approach was to collect, standardize, and organize the objectives of school boards throughout the country into a taxonomy. While certainly not metaphysical in its claims, Bloom's effort would put human behavioral taxonomy on the level of political or moral judgements formulated by local policy makers operating with very pragmatic, local criteria. Travers wishes to propose something between Aristotle and Bloom.

What the middle ground is defined as depends to a large extent on some basic assumptions of the scientific tradition. These assumptions are that theory is "better" than practice; rules and the structures they generate are "better" than habits

and traditions; brains are "better" than minds. No justifications are given save that of the clear success of the natural sciences which assume them. But, Travers is begging an important question: can the natural sciences be a model for the social sciences. An old chestnut to be sure, but Travers argues as though it never existed.

We might view the problem here in the following way. What sort of data should be gathered for the development of a classificatory system of human behavior? If we look at Travers's paradigm example, Mendeleyev's Periodic Table of the Elements, we are struck by its simple organization around the single measure of atomic number. The elements are distinguished from each other by increments of one: hydrogen is the basis with an atomic number of one, helium is two, carbon is six, oxygen is eight, etc. These numbers refer to the numbers of protons in the atom's nucleus. One could, for example, define carbon as six hydrogen nuclei fused together. Can we find analogous measures for human behavior? Can we say that the knowledge of an event or act is a circumscribed functional relationship between discrete measures? To understand a child's concept of time, for example, we would have to be able to reduce perception, recognition, the experience of the "arrow of time" and so forth, as discrete, homogeneous bits of information manipulated by specific, universal, temporally stable rules. Attempts to delineate all this have bogged down in swamps of information, M-spaces, executive schemata and the like. No one doubts the

enormity of this project to operationalize human knowledge and action, but our issue here is can such a project be carried out at all regardless of the amount of knowledge we have, the technological sophistication and the theoretical language to organize and analyze it.

What information bits, homogeneous, culture-free and ahistorical can be used to unproblematically operationalize all human knowledge and behavior so that it could be classified? Informational processing theory and artificial intelligence workers suggest that such data exist, but this remains hypothetical. Moreover, such data must include the discovery (or construction) of rules which govern, in some way, those operations which constitute a particular developmental stage. Emergent from this issue is yet another serious problem: Travers claims that all these actual and hoped-for discoveries are constructs created by the scientist (this is Travers's effort to eschew ontology). But, in doing so, he has raised the issue of instrumentalism. An instrumentalist approach to scientific theory simply argues that an acceptable theory is just the most efficient way of organizing data for research; it is neither true nor is it false. But the assumptions made by Travers's proffered source for a scientific classification of behavior requires a realist point of view: Piaget wants to say that cognitive stages of development are real in some sense.

Travers is expressing some serious confusion when he attempts to wed a pragmatic instrumentalism with cognitive

structuralism. Let me expand on this. Travers argues that classifications, taxonomies and orders are human constructs. Categories must have "some logical structure" or a "theoretical basis," but they do not exist as such. "Classes," Travers says, "with the highest utility [N.B.] were not out there existing in the real world, . . . but were constructions of scientists searching for ways of understanding empirical data" (Travers, 1980: 16). According to Travers, the logic science uses is a formalized version of practical experience. John Dewey, the paradigm thinker for this view, argued that logic is a set of techniques derived by reflection upon the experimental success of certain practices or habits of speaking. Logic, then, in effect, has an inductive base. Travers claims that knowledge is unified, however, and, that that unity is mathematical at its base. It is extremely difficult to make sense of this. Either logic is simply a technique that, like rhetoric, satisfies needs and science is instrumentalist, or logic is a finite set of rules independent of experience and use. With the former, we get a position close to that of Bloom's, namely, the taxonomy should be a synthesis of existing practices, with the latter, we get a kind of Aristotelianism in which the defining structures of cognition exist in some non-empirical way as a depth grammar. In his effort to find a middle ground, Travers has created incoherence.

In another strand of the argument, that of the cognitive status of scientific theories (that is, the question concerning

whether theories are true, false or judged on other grounds), Travers talks as if he were trying to find a middle ground between two opposed philosophical positions by simply confusing them. On the one hand, he argues that classifications are scientific constructs, useful instruments for organizing data. On the other, he must recognize that Piaget wanted to ground his stages of development in physical reality. Structuralists, like Piaget and Chomsky, ignored the difficulty by trying to link formal rules with brain structure. Kant, the founder of structuralist models of the mind, attempted to formulate a priori propositions which ordered experience, but were not dependent on experience: the infamous synthetic a priori. The difficulty Travers faces and the incoherency of his position stems from the ambiguous nature of these structures. Empirical descriptions of human behavior and formal descriptions of the structures "underlying" them are radically different ways of speaking. If we are to avoid arbitrariness and incoherency, we need to formulate a third language which is both formal and empirical, that is, a synthesis of form and content.

These problems are not specifically those of Travers alone; the relation of formal structures and systems and human behavior is wholly problematical. The history of psychology with its pendulum swings between structuralist and behaviorist accounts speaks to this issue eloquently. Empirical tests and experimentation will not help us because such data are dependent upon theories for their very existence

(Glymour, 1980: 110-176). Behaviorist data supports behaviorist theory; behaviorist theory defines action as being analyzable into behaviorist bits. The same relationship exists for structuralist accounts. There seems to be no data that can cut across theoretical boundaries. In physical science, the reality is unified: the universe is made up of leptons, quarks and bosons. Theories of various kinds can be put forward about this reality, falsified or corroborated. But not so in the social sciences. Like it or not, social science cannot provide the empirical unity which would transcend the relativism which appears to be the lot of the social sciences. And relativism at best does not allow us to rationally justify the selection of one theory over another, and, at worst, is self-refuting.

Another way of approaching these issues, suggested by Travers, is the use of modeling one's taxonomy on paradigm cases of successful taxonomies in the more coherent, progressive sciences. The schema Travers admires most is Mendeleyev's formulation of the Periodic Table of the Elements. Here, Travers says, we find a clear example of the kind of scientific work needed for the development of a unified classification for human behaviors. Such an example would be made up of classes of components which are measurements manipulable by standard mathematical procedures; classes related by laws and theories capable of explaining those relations; and, given those laws and theories, unknown classes can be deduced and predicted

from known classes. Finally, all the above are based on sound scientific experimentation. Unfortunately, for Travers, the actual history of this example reveals a very different situation. It is worth our while to consider this history briefly because Travers's justification for his recommendation is dependent on what will be shown to be a misreading of historical reality.

2. Travers's Paradigm Case: Mendeleyev's Periodic Table of the Elements

We will be examining Mendeleyev through Dudley Shapere's reconstruction. Shapere is a philosopher and historian of science who, one can be assured, ought to become well-known among those educational researchers and social scientists attempting to understand their own work through comparisons with historical example. Shapere is neither a positivist, who wishes to simplify science into a logical, axiomitized system, nor is he a relativist, who would argue that science is irrational. His position is generally referred to as historical realism, a position grounded in the history of the practice of science which argues for the reasonableness of the patterns of scientific choices, arguments and conclusions.

In order to appreciate Shapere's discussion, some definitions are in order. Shapere distinguishes between compositional theories of explanation and evolutionary theories of explanation. A compositional theory itemizes parts of a domain, which is a body of information which raises or presents problems

for which a theory gives an account. Such a theory also itemizes the laws which govern the behavior of those parts. An evolutionary theory explains the development of items making up a domain (Shapere, 1977: 534). The domain itself should be made of types of items which are in some kind of order or pattern. Such ordering may allow (but not necessarily) for the possibility of making predictions. There may not be a law, or even a compositional theory, which explains such an ordering.

Mendeleyev was apparently aware of a theoretical insufficiency in his work. He stated that the so-called periodic law is a summary of "the properties of simple bodies, the constitution of their compounds, as well as the properties of the latter which are periodic functions of the atomic weights of the elements." But, he had no theory, or law which could link up the properties of the elements and the measurements of those elements (i.e., the atomic weights). Such a relationship, he thought, had to be mathematical, but he could not provide the formal description of that relationship (Shapere, 1977: 536). Mendeleyev believed that there had to be a law, and he was disheartened by the fact that the predictions based on his Periodic Table were but approximate guesses. There was no "law of periodicity," although the literature of Mendeleyev's time frequently referred to one. By Travers's criteria, the Periodic Table was no more scientific than Benjamin Bloom's. Indeed, the theory which would eventually provide an account was a theory in disfavor among nineteenth-century chemists:

the atomic theory. Mendeleyev argued that this theory was a "relic of the torments of classical thought" (Shapere, 1977: 538). The eventual triumph of atomic theory was a product, not of experimentation, but of expectation. The expectation was a reasonable one given its success in other areas. By the time experimentation supported atomic theory's role, it already had been accepted in scientific accounts. Travers's criteria work more as afterthoughts than as guiding principles of scientific work.

From Shapere's careful historical work, a set of conditions for a classification can be formulated. I will only give a short summary of it as a contrast to Travers's criteria:

The historical evidence indicates that:

--if a domain is ordered, and the order is discrete (that is, it is based on a property which "jumps" in value from one item to a succeeding one), and the items having values are integral multiples of a fundamental item (e.g., hydrogen is "one"), and

--if the order is precise, extensive and detailed, then we can argue for a compositional theory for the domain on the further grounds that other domains have expected compositional theories, that other domains have successful theories and that there is reason to suppose that the domain under consideration is related to other domains to form a larger, more inclusive domain.

The upshot of this is that if Travers wishes to formulate a scientific basis for educational and behavioral objectives, his criteria are simply too rigid. Travers's distorted view of how scientific workers operate can be exemplified by contrasting his criteria with the way other classifications were formed. Scientists in the nineteenth century worked on the supposition that there was an analogy between atomistic and molecular vitrations and sound waves. They had no theory to support this analogy; scientists simply felt that since a particular model had worked well in one situation, there was no reason to suppose that it would not work in another. Shapere concludes this discussion by noting that the constancy of discrete values of one situation could be carried over into that of another "even though no general formula (or qualitative principle) expressing the relations of these values is known" (Shapere, 1977: 546).

Clearly, if we look at scientific practices, we find a mixture of intuition, "reasonable" supposition, and expectation. In one respect, Travers's selection of Dewey's description relating logic to a practical basis is close to Shapere's account here. But, what does not follow is a Deweyan instrumentalism, that is, the idea that what is being generalized from specific data is not real, but is only a construct. Moreover, Travers's own discussion does not lend itself to the selection of structuralism as a "sound" basis (contra instrumentalism) for a taxonomy of human learning behaviors. What

any such structuralism presupposes is the existence of rules, which occur (in some fashion) prior to actual behavior. The behavior, in turn, helps to revise or reconstruct the rules. How this revision takes place is a problem we will turn to in the next section. Clearly, the examples of Travers's choice do not support his contention that experimentation, "systems of measurement," prediction and laws always exist in the formulation of scientific taxonomies.

3. The Problem with Structuralism

Travers's essay ends with the hope that a scientific taxonomy is on its way. It is merely a matter of understanding what such a taxonomy implies. Once it is in place, the evaluator may then have, in effect, a common language for describing human learning behavior. Indeed, all of empirical science would have a neutral data base with which it might test its theories. Travers's puts forward Piaget's genetic epistemology as an example of a research program which may provide the basis for such a classification system. Unfortunately, Piaget and other structuralists have some serious, possibly insurmountable, problems. In fact, these problems are similar to the difficulties Travers has in attempting to eliminate the need to make an existential judgment about theories. The problems are:

--the relation between formal (rules, depth grammars, generative grammars) and informal (surface behavior, interpersonal behavior, practices) levels is extremely vague;

--behavior classification based on rules or executive schemata operant at any given stage of development would be content and context-free while the behavior purportedly explained is neither;

--surface behavior, including experiences of the world and other persons, would have to be redefined as bits of discrete, standardized information manipulable by abstract rules which raises the problem of the accuracy of translation from a language of actions to a language of information processing;

--rules of a transformational grammar are themselves dependent on practices and "prior" knowledge of, for example, what an object is, what it means to have a body, how the body occupies and takes a stance in relation to other bodies in a public space.

These are issues which are rendering structuralism problematical. Indeed, Travers makes them more so with his instrumentalism: he admonishes us against thinking that structural elements are really "out there." As he says this, he also says that classifications in the phenomenal (surface?) world are not easily parallel to relations in the real (depth?) world. Interestingly, he notes that while events are causally related to other events, thoughts, as phenomena, are not causally related. Phenomena are ephemeral observations which can provide no genuine classifications. Reality is causally determined, that is, it is lawlike, predictable, and universally

identical as a structure. All of this is nothing new; it is the modern, scientific version of the old reality vs. appearance dichotomy inherited from Plato. The Forms of Plato are "more real" than the world of mere experience. Travers's espousal of this dichotomy, also consistent with structuralism, is all the more remarkable given his rejection of metaphysics. Travers is one with other social scientists in that they are blissfully unaware of their own metaphysics, thinking that this demon might be exorcised by the wave of an empiricist wand.

Structuralism came out of the Kantian position that metaphysics could be replaced by epistemology. That is, the categories and levels, essences and Absolutes of the metaphysician could be replaced by the structure of the mind as a cognitive, a priori system. Ironically, his position gave birth to some of the wildest flights of metaphysical fancy as philosophers attempted to understand just what sort of thing this "mind" was. Piaget sought to avoid this by showing that new rules for knowing reality were constantly being constructed. This construction is analyzed into two moments, accommodation and assimilation. The child, struggling with contradictions between what s/he thinks is the case and what clearly is the case, resolves the contradiction through the development of more inclusive, precise and reality-tested structures. A surface/depth distinction is essential to this picture. The new rules are not all Deweyan habits abstracted into logical rules; they are systems of rules which determine knowledge of

the world. The child slowly moves to the point where s/he can finally understand the crowns of human knowledge and reason: physics and mathematics. The child also gains knowledge of a world of causes, formal operations, and a social recognition of the points of view of other persons. Once more we see the Platonic bias towards the theoretical, non-experiential, contemplative over the practical, traditional, morally constitutive world of everyday life.

By eliminating the inconsistent, indexical, culture and situation-bound world in favor of formal universal rules without history, we eliminate most of the ways people ordinarily understand themselves and their world. Self, community, political order, intimacy, belief, etc., all become "folkloric" to be replaced by scientific rules and theories. Indeed the very idea of a self has been specifically targetted as "prescientific" (Searle, 1985: 28-41; Turkle, 1984: 167; Stich, 1983; for the social implications of this, see Dreyfus, 1981: 507-581). If we abandon the idea of a self, however, we eliminate the moral importance of social relations, choices, decisions, and political and religious traditions. The importance of these larger contexts to understanding language, for example, is becoming clear in the work of Terry Winograd. Computers cannot process natural languages because meaning, semantics, is fundamental to determining precisely what a phrase or a sentence uttered is referring to, evoking or expressing. A simple statement like "The chickens are ready

to eat" is an undecidable proposition for a computer. Only a person in a context with some notion of precedence and consequence in a lived situation could understand it. A computer is a physical realization of a formal operation which would render most human language, which is social and interpretive, unintelligible.

The importance of context and content in language understanding has enormous consequences for the claim that computers think or the related claim that human thinking is identical to what computers do. "Virtually every word has an open-ended field of meanings that shades gradually from those that seem utterly literal to those that are clearly metaphorical" (Winograd, 1984: 135). Since education is still largely a social relationship between persons, the context and content of language use stands as a significant stonewall to the claims made by Piaget, Chomsky and Travers about the superiority of a classification which is neither social, authoritative, morally exemplary, nor evocative of the traditions which make being human valuable. A behavioral classification based on Piaget would be wonderful for evaluating computer programs.

Whenever we speak to another person, we embody and express a shared background of practices, beliefs, expectations, institutions and memories. This background is learned in a preconscious way by the example presented to us by perspicuous authority figures in our lives. Learning takes place in social situations: the family, the neighborhood, the school; it is

conveyed by friends, family relations, popular media in self-conscious ways and in peripheral, unthought ways. Such relationships and forms of communication involve moral and political dimensions, even on the most microscopic levels of everyday practice. Everyday practices are filled with negotiations, compromises, resistances, disputes, sharings, and interpretations. Such practices embody norms of authority, power, truth, deception, and everything else that we experience, but must scrupulously avoid if we adhere to structuralist theorizing.

The structuralist account claims that abstract operational rules and procedures have precedence over all of these situations. To accept a structuralist account, then, means that a rule, abstract and universal, existing in a Platonic heaven, can be used to explain learning events because these events "follow" the rule. The rule and the event are locked in an ambiguous relation which comes close to being circular.

Structuralists are at a loss to explain what this relation between rule and learning event is exactly. Is this relation itself rule-like? If it is a rule, does it have content or not? If not, how do we distinguish it from the explanatory rule? How is <u>it</u> related to the learning event? If the connecting relation has content, is it therefore not as universal as the explanatory rule? If it is not as universal, then are there other "near-rules" operant in other situations? How are <u>these</u> connecting rules linked to the explanatory (purely

formal) rules? In short, advocates of formal rules explaining informal, situated actions have the same sorts of problems Descartes had when critics demanded an account of the relationship between immaterial mind and material body.

One increasingly important answer to these difficulties should be mentioned briefly. Namely, the identification of transcendental rules with brain functions, states or events. Information processing, expressed as electrochemical shifts of energy in the nervous system, which all humans have, seems to give life to bloodless categories. It also explains, in a materialist way, transcultural validations of cognitive development. But, while such validations and explanations are important, they still lack the semantic and cultural conditions which allow or enable learning or acting to take place. Physiological depictions of human events have to be shown to be "equal" with cultural depictions of human events. The latter are normative, consensual and intentional. Since we do not have a unified theory of rationality (and we are not likely to get one) we have no rational perspective from which we might choose one depiction as the best possible account. If we decide that the physiological and the cultural are simply parallel languages, then the question of the nature of the relations between them emerges in full force.

Structuralist accounts, therefore, cannot take into consideration cultural, historical, and intentional elements of learning and recognition. Certainly, these accounts may

provide part of the picture, but we cannot say whether the part explained by structuralist accounts is necessary or contingent to the learning process. Indeed, it may be that culture provides information about the world which rule-governed, or generated, structures must have in order to function at all. Even if we manage to associate (identify?) such rules with brain states, and if we may be able to predict future brain states, we still could not predict particular verbal, or non-verbal, expressions, symbolizations or other forms of communication. Brain states are responsible for large numbers of these human events, but the significance, value and purpose of these events are unpredictable because the possible number of interpretations is virtually innumerable. If we can say that brain states x, y and z are responsible (cause?) the carving of the Pietà, that cannot account for the beliefs and meanings intentionally, and pre-consciously, ascribed and identified with the physical object produced, its lines, the position it holds in space, its evolving saliences and peripheries, and the place it holds in human consciousness. One might as well try to deduce all this from its weight.

Even if we are able to find law-like bases of human learning, mathematically manipulable, experientially backed and predictive, we would not be able to identify the relevant events in fluctuating, polysemic and--dare I say it?--holistic social situations. This is significant not only for evaluation and educational research, but for the empirical social

sciences. A deeper reason for the futility of Travers's project exists which underlies not only why a taxonomy for behavior does not exist, but why one cannot exist: human behavior is unpredictable.

4. Can human beings and their actions be predicted?

Thus far, human actions have not been predictable in the sense that they follow from highly probable, universal laws. Without such laws, any alleged predictions are more like rules-of-thumb, or a highly articulated form of common sense. In effect, the only claim which can be rationally made about a successful prediction is that a given hypothesis is consistent with human action. The question of whether this consistency is due to an underlying law, or that there is simply a congeniality between the researcher's background of practices and beliefs in the background of the "subjects" is constantly begged. These issues cannot be dealt with adequately in this paper. I can, however, present some arguments which have emerged in recent writings that have cast a pall over any remaining hopes that prediction of human actions is at all possible.

One argument for the unpredictability of human actions is clearly based on the failure of the social sciences to evolve a stable basis of knowledge and theory. There are no social science equivalents to evolution, atomic theory, or gravity. Moreover, there is no evidence for the gradual and progressive elimination of theories in favor of more inclusive ones. The

physics and biology of Aristotle, for example, are largely historical artifacts, but his politics and ethics remain compelling and influential. The nature and structure of human society, action and motivation is as much a field for philosophy as for the most empirically inclined researcher.

With prediction reduced to the level of numbers gathered under certain conditions functionally related to other numbers gathered under similar conditions, values have been impossible to expunge as they have been in physics. Correlations in physics have always been some orders of magnitude greater in predictive accuracy than in the social sciences. In addition, laws can be referred to as explanations for why the correlations should be taken seriously, that is, as potential causes. In the social sciences, theoretical positions are adhered to not because of their factual support, but, rather, because they express certain desirable or valued ends which those theories suggest as basic to human relations. To be more specific, the social sciences have been individualistic, ahistorical, and congenial to governmental interventionism. Ideology, even in a benign sense, is never far from the surface "rigor" of social science. This conclusion is equally true for other, alternative, social science paradigms; Marxism is the obvious case.

If a social science is to emerge which is the epistemic equivalent of a theory in the natural sciences, then much of what is evident in daily life would have to be systematically

eliminated: self-interpretation, the constant re-understanding of actions in terms of historically embedded meanings. In short, human actions would have to be reified as the relations between objects. And this only for the ease of the researcher. But those actions, routines, practices, meanings and beliefs are in a constant state of fluctuation. Once removed from the give-and-take, dialogue and re-constitution of everyday life through language, human action can be predicted. The difficulty comes back when the researcher attempts to return his findings to human reality in the form of replication or policy. He or she finds that the corroborated hypothesis has degenerated into something surprising.

By removing actions and practices from the self-forming narratives of everyday life for the purposes of making a particular methodology work, we are in danger of producing knowledge which is more of an artifact, than the hoped-for objective "mirror" of society.

Practices have another aspect to them which further precludes prediction. Actions have moral and political content. Michel Foucault's work indicates that even the most benign activity of statisticians, census taking, is part of a larger phenomenon which facilitates the political control of whole populations. Classifications of mental disorders, social groups, educational abilities and differences, etc., are a means for politically pacifying the population. The avalanche of numbers phenomenon (Hacking, 1982) is part of the energence

of a disciplinary society which aims at bureaucratic control of the population. In the nineteenth century, when the effort to rationally control humans began in earnest, categories were invented and re-invented with an almost baroque elaboration of detail. Categories were invented to explain and define and predict the actions of people with "split personalities" or who were members of a "criminal element." The same work, with more elaborate methodologies at work, might be exemplified by the arguments pressed by the American Psychiatric Association for the World Health Organization's adaptation of standardized classification of mental disorders. "It is illegal," says Hacking, "to die in any way except those listed in the WHO nosology, and if the APA has its way, it will become illegal to go mad except according to the classifications of DSM III" (Hacking, 1982: 294; for the creation of "split personalities," see Hacking, 1983).

In effect, the applicability of categories, methods of predicting behavior, etc., will become not neutral forms of inquiry, but, rather, the instruments by which practices become incorporated into bureaucratic policy. Scientific research, even of the most rigorous kind, becomes a social event. Studies, experiments, the creation of a behavioral taxonomy, all become culturally imbued with meanings whether intended or not. Science has become one of the practices which shapes social behavior. Predictions of such behavior cannot help but become "self-fulfilling," or become symbols consciously

resisted. Therefore, one of Travers's categories of behavior (social or educational) will have to be "scientific prediction," "theory building" and the various responses, positive or negative, to it. Being able to predict these categories would involve predicting future scientific work and public responses. Certainly, even the most critical reader of this paper should see the difficulty with that. Moreover, there is a growing literature indicating that human subjects involved in experiments are motivated primarily by self-image management. If the experiment hoped to corroborate or falsify some hypothesis on human motivation, then any self-image management would invalidate the experiment regardless of results. Experiments are social relations. People (even if we call them "subjects") are going to respond to the experimenter, a fellow "subject," or an empty room in radically different ways depending on his/her social relations, personal history, values, and self-regard (Hales, forthcoming).

Conclusion

Human beings' capacity to transform their actions into stories which make sense to them underlies the objection to the attempt to translate human activities, routines, practices, etc., into finite, denumerable classes of information. Prediction depends on the elimination of such a narrative function along with traditional notions of "self," "individuality," "society," "community," "value," "commitment," "intention," and "meaning." Any classification which is more than a mere

compendium of what happens to be in effect at a given point in time (which was Travers's charge of "unscientific" to Bloom) must accept something like prediction, or the translation of practices into classes of information and their criteria or rules. Or, put another way, a scientific classification system would have to eliminate much of what constitutes human life in order to be scientific. Even if Travers finds a way of eliminating the radical distinctions between the Kantian formalism of Piaget and the pragmatic experimentalism of Dewey (both of which he holds), he would be hard-pressed to devise a classification scheme that had anything to do with the way teachers and children live together in the school.

Travers is not at fault here, however. He is simply attempting to extend a version of scientific thought to an area which many would consider to bein dire need of scientific rigor. In order to do that, Travers would have to resolve rationally the question of the cognitive status of such a clasification (is such a classification "real" or a "construct"?). He would have to establish the reality of such a classification independently of cultural, social and historical settings. A scructuralist approach, such as Piaget's, claims to do just that; there is, however, no clear way that such a formal grammar could be related to human behavior. Moreover, the rules and structures which purportedly explain human practices themselves presuppose practices which, in turn, are in need of explaining. Thus, structuralism fails.

Even if we abandon structuralism, one could argue that prediction is still a compelling goal for social scientists to strive for. Unfortunately, the logic of prediction requires a law to be at least assumed, otherwise one has only an inductive aggregation (the aggregation itself dependent upon an interpretation) of the "All swans hitherto have been observed to be white, hence, the next swan has an observed-swans/100 chance of being white" variety. The failure of the social sciences to produce anything more than inductive aggregations (which degenerate over time anyway) speaks volumes against the discovery of laws which might support predictions. The more likely position is that correct predictions are more like coincidences or congenialities of hypothesis and observation, than they are scientific inferences.

Any attempt to formulate a taxonomy of human behaviors outside of human historical contexts is bound to be a dead-end; any taxonomy bound by historical context will, eventually, become irrelevant.

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